Review of Revised

The conflict between sampling resolution and stratigraphic constraints from a Bayesian perspective: OSL and radiocarbon case studies

20th Jan 2026

Overview

I think this version is much improved compared with the previous version. I understand much more clearly the points that the authors are trying to make:

1) That when you have a set of samples that are very similar in date then, if fitting joint Bayesian models with the specific priors described in Eq (1), the posterior will seemingly tend to spread out these dates (which may be unwanted). This is what they call the spread effect.
2) That through modelling of phases (in OxCal in particular) one can concentrate dates (i.e. shrink them into narrow intervals) – the concentration effect

In my view this is likely an innate property of the prior in Eq 1 (or priors in the case of the phase model). No prior is actually uninformative in any Bayesian model, and perhaps the Eq 1 prior actually prefers some spacing between dates? This is not necessarily a failing but a property of the prior that perhaps people do not realise. As a simple example, in the case where we only have two dates in Eq (1) then you can directly work out the equivalent prior on the difference in ages, $d = t_2 - t_1$, and it is not uninformative (although interestingly I might have expected it to shrink the dates towards one another).

Key Message

I certainly agree that the fundamental message for readers should be that all users of Bayesian models must check their assumptions and the suitability of the specific model and prior that they are fitting (and not just treat them as a black box).

I do also agree that in the Jianbing examples provided it seems (visually) to be the case that the model posterior has overly spread the dates at depths 200-300m. However, I feel that readers should maintain some caution when reading the broad interpretations provided which, IMO, are quite strong in places regarding where the model posteriors are wrong. The authors have a tendency to perhaps present their interpretation of the posteriors (and their failings) as definitive whereas, as we do not actually know the right answer, some of these inferences are perhaps a little more subjective.

This OSL example (and to some extent the Catalhoyuk example too) are interpreted/criticised based on the authors implicitly assuming that they know what the right answer is, e.g., for the OSL ages that effectively they seem to think there is a large and sudden accumulation of sediment from the same calendar age. This is entirely fine/legitimate, but it might be considered simply to be a different subjective prior (quite possibly a better prior if people think there are such fast accumulation processes). Importantly, readers should note that they do not show any examples where the models actually might work to improve inference.

I noticed a few minor typos that need revising (and I have suggested a few further minor comments that IMO would be more equitable in terms of accompanying interpretation). Once the typos are fixed then I would accept the paper (I will leave decisions on the more subjective commentary suggestions to the editor/authors):

Typos/Lack of Explanation

1) Line 47 – 54 (Baylum model) – No explanation given of what e is. I would also argue this is not the same prior as Eq (1) due to the introduction of log, assuming t are the actual dates of interest (rather than e)?
2) There is some odd bullet pointing tat extends into the margin throughout my version (e.g., line 50, 95, …)
3) Line 213 – Lost capitalisation in OxCal (not oxCal)
4) Figure 2 caption refers to section 3.1 below (when discussing outliers). The only mention of outliers I could find was on line 214 (i.e. Section 2.2 and not 3.1). Does this Fig reference need updating?
5) Line 507 – a spurious line break has been introduced in the middle of a sentence

Comments (not required for acceptance but I would personally reword):

1) I would reword (tone down) the text c.a. line 427 about the start of the mound not being credibly before the date of the earliest sample and that one should not extrapolate. To me this is a highly subjective statement, and implicitly assumes substantial extra information about the sampling, i.e., that this mound is so heavily sampled that the oldest object must in fact be from the very date that the mound was created. I think it is fine to say something similar to what is there currently, but without the definitiveness that suggests extrapolating is wrong.

   In many instances, I think it is quite plausible (indeed would be correct inference) to have an estimate for the date of the start of a phase (e.g., mound creation) that extends before the first/oldest sample on the basis that you probably haven't dated a sample from the mound creation. This also goes in the other direction, where an extinction/mound end can be considerably after the most recent bone/sample has been found, i.e., the Signor-Lipps effect: https://en.wikipedia.org/wiki/Signor%E2%80%93Lipps_effect

   In fact I actually think this is something that a (good) model might actually be able to inform you on – if you have very dense dates for samples in a mound (i.e., all close to one another) one might (??) think that they do mark the full use of the mound and so the earliest and latest dates should be v near the mound creation/end. However, if you only have a few samples (that are widely spread in calendar age) you might worry that the start of the mound could have been considerably earlier and you just haven't sampled much.